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The ABCs of Who Benefits from Working with AI: Ability, Beliefs, and Calibration

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Abstract. We use a controlled experiment to show that ability and belief calibration jointly determine the benefits of working with artificial intelligence (AI). AI improves performance more for people with low baseline ability. However, holding ability constant, AI assistance is more valuable for people who are calibrated, meaning they have accurate beliefs about their own ability. People who know they have low ability gain the most from working with AI. In a counterfactual analysis, we show that eliminating miscalibration would cause AI to reduce performance inequality nearly twice as much as it already does.

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1. Introduction

In the future, many workers will use AI tools. How will these tools affect productivity and inequality in the labor market? Broadly speaking, AI is trained on a vast corpus of human examples and can perform a variety of tasks at, or near, the level of human experts. But AI rarely outperforms the best humans, and workers can complement AI both with contextual knowledge and the ability to deal with atypical examples. In this paper, we propose that complementarities between workers and AI tools will be greatest when workers have an accurate appraisal of their own abilities.

We show that workers with well-calibrated beliefs make better use of AI assistance. Calibrated beliefs are those that are aligned with objective likelihoods. For example, when 90% sure, a calibrated person is correct 90% of the time.[1] Belief calibration is important regardless of the AI's capabilities. Consider the case of an AI that provides assistance to workers who are making some prediction about a future event, such as whether a financial transaction is fraudulent or whether a person will reoffend if released on bail. If the AI's prediction is very poor, underconfident workers will defer too much even if they have better information. If the AI's prediction is extremely accurate, overconfident workers will override it when they should defer, limiting how much can be gained from AI assistance (Hoffman et al. 2018).

In this paper, we introduce an experimental framework to measure both ability and calibration and their impact on the gains from AI assistance. To measure these characteristics properly, we must elicit probabilistic assessments from a large sample of participants in a task for which there is an objectively correct answer (a clear "ground truth") and with an appropriate level of task difficulty relative to a calibrated AI.

We ask 732 experimental participants to report the likelihood that 160 different people in photographs are over 21 years of age. Some participants are randomly assigned an AI assistant that provides them with an algorithm's probabilistic confidence score. The AI assistant is more accurate than the average person but less accurate than the best person, a fact that is known to participants. Treatment group participants receive AI assistance for only some images, which allows us to estimate baseline ability at the task. We also measure general cognitive ability by administering a short form of the Raven's matrices test.

We find that AI assistance improves prediction accuracy on average and with heterogeneous impacts across individuals. We present three main findings addressing the question of "who benefits?" First, the impact of AI assistance depends on whether participants are calibrated about their own abilities. Holding baseline ability fixed, a one-standard-deviation increase in

calibration increases the average treatment effect of AI assistance by 20%.

Second, we find that participants with high calibration *and* low ability gain the most from working with AI. This is consistent with the finding that AI closes the performance gap between lower performers and their more expert peers (Noy and Zhang 2023, Autor 2024, Brynjolfsson et al. 2025).

Third, we find that miscalibration limits the extent to which AI assistance can reduce differences in productivity ("performance inequality"). In a counterfactual analysis where miscalibration is eliminated, the productivity gap (captured by interquartile ratio, a measure of spread) would shrink nearly twice as much as it does already with the introduction of AI assistance. The reason is that low-ability participants also tend to be more miscalibrated, which limits the benefits from working with AI.

Our paper makes two main contributions. First, consistent with several recent studies, we find that AI tools increase average productivity and reduce disparities in output between high-skilled and low-skilled workers (Gruber et al. 2020, Noy and Zhang 2023, Choi et al. 2024, Merali 2024, Brynjolfsson et al. 2025). For example, Gruber et al. (2020) find that giving health insurance brokers AI-based decision support improves retiree choices by $278 on average and decreases performance heterogeneity among brokers. We extend this literature by showing that belief calibration may be important for understanding when AI tools will meaningfully reduce productivity differences.

Second, we extend the experimental literature on the impact of AI advice on human decision making, particularly those studies that find a relationship between confidence and willingness to rely on AI advice (Tejeda et al. 2022, Dargnies et al. 2024).[2] The closest paper to ours is Agarwal et al. (2023), who find that the impact of AI on decision quality is influenced by the relative confidence of humans and machines. In Agarwal et al. (2023), radiologists reported how likely chest radiographs were to exhibit different pathologies, with and without AI assistance. AI assistance improves performance in cases where human radiologists are relatively uncertain, but performance suffers when humans are more confident, leading to a zero average effect. Our paper builds on this work by making three methodological innovations: we collect a large number of observations from each individual, implement a task with a clear ground truth,[3] and use task behavior to measure belief calibration. Together, these innovations allow us to disentangle the separate impacts of ability and calibration. We show that individuals with better-calibrated beliefs (before receiving AI assistance) benefit more from AI assistance and that this impact is especially pronounced for individuals with low ability at baseline.

The paper is structured as follows. Section 2 introduces a minimal conceptual framework to define our main measures of ability, beliefs, and calibration, as well as our empirical strategy. Section 3 discusses the experimental design and participant pool. Section 4 presents our experimental analysis and results. Section 5 concludes with a discussion.

2. Ability, Beliefs, and Calibration

2.1. Measures

An individual decision maker i faces a number of binary classification task instances $\omega \in \Omega = [0,1]$ for which we elicit probabilistic beliefs $B_i(\omega) \in [0,1]$, henceforth encoded in random variable B_i. True states across task instances are summarized by a random variable $S \in \{0,1\}$ with balanced prior $E[S] = 1/2$. We use the relationship between an individual's beliefs B_i and the true states S to define a simple measure of both the individual's *ability*—what they know—and *calibration*—how well they know what they know. In words, we measure ability as how likely an individual is to be correct if they always guess the state they believe is more likely to be true, confidence by how likely they believe they will be correct, and calibration as the absolute value of the difference between the two.

More formally, define a random variable summarizing whether beliefs would have induced correct choices at a decision threshold of 50%:

$$R_i = \begin{cases} 1 & \text{if } |B_i - S| < 0.5 \\ 0.5 & \text{if } B_i = 0.5 \\ 0 & \text{else,} \end{cases}$$

and define the individual's ability as the expected accuracy of such choices:

$$A_i = E[R_i]. \tag{1}$$

For calibration, define a random variable summarizing the confidence of beliefs over each image:

$$K_i = \max\{B_i, 1 - B_i\},$$

and define the individual's net confidence as the expected difference between their confidence and correctness:

$$N_i = E[K_i - R_i].$$

If correctness exceeds confidence $N_i < 0$, we say that an individual is underconfident; if confidence exceeds correctness $N_i > 0$, we say that an individual is overconfident. In either case, the larger the difference between perceived and actual performance, the less an individual's beliefs are calibrated. We capture this notion by defining the individual's calibration as the negative absolute value of their net confidence:

$$C_i = -|E[K_i - R_i]|. \tag{2}$$

Empirically, an individual is calibrated if, say, expressing 75% confidence in their classifications, they are also correct 75% of the time. Alternatively, calibration

could be further disaggregated across bins of an individual's confidence (DeGroot and Fienberg 1983) or estimated parametrically (Grether 1980); in Online Appendix A.4, we show that our results are robust to such alternative definitions of calibration and to alternative definitions of ability, such as the area under the receiver operating characteristic curve, or AUC (Bamber 1975, Hanley and McNeil 1982).

Our main question is how ability and calibration relate to the ability to incorporate AI advice. In contrast to human decision makers, we ensure that the AI model's probabilistic predictions, which we denote by M, are calibrated. Working with the AI, individual i observes and integrates the AI predictions M before reporting their AI-augmented subjective beliefs $\overline{B}_i$, which determine AI-augmented ability $\overline{A}_i$ as in (1) and calibration $\overline{C}_i$ as in (2).[4] If the human benefits from the AI but also does not repeat its predictions M wholesale, we expect that the AI-augmented human beliefs $\overline{B}_i$ will differ from the human beliefs B_i and the AI beliefs M on their own.

To state our main prediction, we consider a population of individuals summarized by a joint distribution of individual and AI-augmented abilities and calibration $(A_i, \overline{A}_i, C_i)$. Our main prediction is that, conditional on their individual ability A_i, individuals who are more calibrated will benefit more from working with AI in terms of their AI-augmented ability $\overline{A}_i$. Formally,

Prediction 1. *AI-augmented ability* $E[\overline{A}_i | A_i = a, C_i = c]$ *is increasing in calibration* c *given ability* a. *Equivalently, AI value-added* $E[\overline{A}_i - A_i | A_i = a, C_i = c]$ *is increasing in calibration* c *given ability* a.

The equivalence of the level and value-added interpretations is immediate, given the conditioning on ability. However, conditioning on ability is generally important because the net confidence underlying calibration (2) is a linear function of accuracy.

2.2. Empirical Framework

We now embed our main prediction into a standard causal framework. In a random subset of task instances we call the control block, all individuals work without AI assistance. We estimate individual skills A_i and C_i for each individual i using plug-in estimators of the respective Definitions (1) and (2) in the control block of task instances. In the remaining subset of task instances we call the treated block, we randomly assign as treatment $T_i \in \{0, 1\}$, whether human i worked with or without AI assistance. To distinguish outcomes from skills, we adopt potential outcome notation: let $Y_i(0) = A_i$ denote individual i's performance if (potentially) assigned to work alone, and let $Y_i(1) = \overline{A}_i$ denote individual i's performance if assigned to work with AI. The treatment effect $Y_i(1) - Y_i(0)$ is thus the value-added $\overline{A}_i - A_i$ from working with AI. Let $Y_i = Y_i(T_i)$ denote the observed outcome given treatment status. We estimate individual performance Y_i for each individual using a plug-in estimator of (1) in the treated block of tasks.

We test Prediction 1 in a linear regression specification:

$$Y_i = \alpha_0 + \alpha_1 A_i + \alpha_2 C_i + \beta_0 T_i + \beta_1 A_i T_i + \beta_2 C_i T_i + \varepsilon_i. \quad (3)$$

For intuition, first consider the case where there is no heterogeneity in skills (A_i, C_i). Then, α_0 captures the average ability of individuals working alone and β_0 the average treatment effect of working with AI. In turn, the remaining coefficients $\alpha_1, \alpha_2, \beta_1$, and β_2 capture observed heterogeneity in the level and treatment effect in terms of ability and calibration, respectively. Our main Prediction 1 is that the treatment effect is increasing in calibration c given ability a, corresponding to a significantly positive coefficient $\beta_2 > 0$ on the interaction of calibration and treatment in Regression Specification (3). Details on the research design and regression specification are summarized in Table 1.

2.3. Measurement Challenges

Our experiment will allow us to estimate individual ability and calibration from observed beliefs, working with or without AI. Nevertheless, estimation and use of these measures raise several challenges. For each participant, we need to elicit a large number of probabilistic beliefs to estimate ability and calibration skills and to reduce measurement error therein. We need to do so in a task with (i) a clear, objective ground truth, and (ii) an appropriate perceived and actual difficulty so that the distribution of probability reports and latent beliefs is not skewed toward extremes. Our focus on treatment effect heterogeneity requires this for a large sample of participants and is furthermore predicated on the presence of heterogeneity in subjects' calibration, conditional on their ability. Additionally, for working with an AI, we need AI predictions to be well calibrated over the selected images and to be of a suitable diagnostic ability. We next describe our experimental design for surmounting these challenges, beginning with the experimental task.

3. Experimental Design

3.1. The Task

In each round of our experiment, participants were presented with an image of a human face and were asked

Table 1. Basic Experiment Design and Coefficients Estimated in Equation (3)

	Control Subjects	*Treatment Subjects*
Control Images	Estimate A_i, C_i	Estimate A_i, C_i
Treatment Images	Estimate $Y_i(0) = A_i$	Estimate $Y_i(1) = \overline{A}_i$
Effect	α_1, α_2	$\alpha_1 + \beta_1, \alpha_2 + \beta_2$

to report the probability that the individual was over 21 years old at the time the image was taken. Participants were informed that all images were taken between 2010 and 2014, with half of the images depicting individuals who were under 21 years old and the other half depicting individuals over 21 at the time the image was taken.[5]

In half of the rounds, participants in the treatment group were also shown an "AI Assistant guess," which was an AI prediction of whether the individual was over 21 (a percentage between 0 and 100). Participants were told that the AI Assistant guesses were more accurate than the average human but worse than the most skilled human. See the Online Appendix for screenshots of choices with and without AI predictions.

3.2. Implementation

We recruited participants from Prolific, a popular platform for conducting online experiments. Prolific requires participants to be at least 18 years old, and we further restricted participants to be U.S. residents. In the recruitment material, participants were told that they would be given $5 as a participation fee if they finished the experiment and that they could receive additional bonus payments based on their choices in the experiment.

After being shown a consent form that outlined the basic conditions of the experiment, all participants were asked whether they would like to participate. Our experiment was deemed exempt from the Federal Regulations at 45 CFR 46.101(b) by the Human Subjects Committee at the University of California, Santa Barbara (protocol number 11-22-0691). Upon agreeing to participate, participants answered two unincentivized questions as an attention check. Participants had only one chance to answer these questions correctly to proceed with the rest of the experiment.

The rest of the experiment began with several pages of instructions, which provided an estimate of the session length (30–35 minutes), the possible bonus payments ($5 and $1), an explanation of the task they would complete in each round, the total number of rounds (160), and how the bonus payments would be awarded. To incentivize the truthful reporting of beliefs in the task, we used the same incentives provided by Agarwal et al. (2023). A random round was selected (all were equally likely to be selected), and based on the reported probability and true age, the binarized scoring rule (Hossain and Okui 2013) was used to determine the probability of getting the $5 bonus payment. Following Danz et al. (2022), we provided few details about the scoring rule but indicated that the likelihood of receiving the bonus was maximized by truthful reporting of their belief in each round.

After the experimental instructions, participants completed four unincentivized practice rounds. Participants were not provided with any feedback about their choices or the true age of the individual during the practice rounds. After the practice rounds, participants faced 160 incentivized rounds, divided into eight blocks of 20 rounds each. There were opportunities to pause between blocks, which were intended to mitigate fatigue effects. Participants were also not provided feedback during the incentivized rounds.

In these rounds, participants were provided with a slider bar that ranged from 0 to 100 for reporting the probability that the person in the image was over 21 years old. To minimize anchoring effects, the slider bar did not have a starting location. Participants could click anywhere on the bar to select a probability and adjust it before clicking the submit button to record their choice and proceed to the next round. They could not go back to previous rounds once the submit button was pressed, and they were given a time limit of 60 seconds for each round. Subjects were informed in the instructions that they would have no chance of winning the bonus payment if, in the round that was randomly selected for payment, they had not submitted a response within the time limit.

3.3. AI Assistant

Participants in the control group were not shown any AI Assistant predictions during the 160 incentivized rounds. For participants in the treatment group, AI predictions were provided in blocks 2, 3, 5, and 8, which correspond to the treated blocks. The arrangement of treated and control blocks was chosen to minimize differences in fatigue between treated and control images and to minimize order effects. In each session, images were assigned to be in a control block or treated block, so we refer to images as either being treated or control images. Whereas images were fixed across participants in a session to be either treated or control, within participant, the images were randomly assigned to a random round within the block.

Participants in the treatment group were not told in advance that they would be given AI predictions. Instead, after completing the first block of rounds, participants in the treatment group were told that they would be shown an AI Assistant's guess for the upcoming block of 20 rounds (the first treated block). In addition to being told that the AI Assistant performs better than the average human but not as well as the best human, they were also told that the AI predictions were calibrated, and they were given the option to learn what it means for the AI to be calibrated.

For our AI predictions, we used the "Caffe" model trained by Rothe et al. (2018), which predicts human ages based on facial images.[6] To generate a confidence score that the person in an image was over 21 years old, we summed the confidence scores for all ages above 21.

3.4. Extra Tasks

Following the 160 rounds, participants completed two additional tasks. The first task was to complete 14 questions from the Raven's Progressive Matrices test. This task was incentivized by paying the participant a $1 bonus if a randomly selected question was answered correctly. The second task was a brief survey in which participants were asked to describe their experience in the experiment, indicate the point at which they felt fatigue during the experiment, and provide any other feedback.

The final part of the experiment consisted of result pages for the incentivized tasks. As in Caplin et al. (2020), we attempted to make the random outcomes used in payment more credible by using the machine time at the moment the participant pressed a button.

3.5. Design Choices

In this section, we discuss several of the main decisions we faced while designing the experiment.

Task Choice: We chose this age classification task for several practical reasons. First, it is a task that can be completed quickly and with minimal effort, allowing us to collect a large sample of participants and a large number of reports per participant. Second, we could easily modify the task's difficulty by varying the ages included in the experiment. For example, ages closer to 21 are generally more difficult to distinguish than those farther away. We set the level of difficulty so that the best participant could outperform the algorithm, whereas the average human performance remained below it, as in Agarwal et al. (2023).[7] Third, this task requires no specialized training, as it originates from a real-world setting. Common examples of guessing whether someone is under or over 21 include scenarios involving liquor sales, such as stores, bars, restaurants, and entertainment venues. Additionally, people unconsciously judge ages in their daily lives. Thus, age classification does not require specialized expertise and is well suited for an online experiment to collect a substantial amount of data. Fourth, this task has a clear ground truth: the age of the person in the photograph. Our coarse classification scheme provides further protection from small amounts of measurement error in the recorded age.

In addition to these practical strengths, we argue that our simple task has structural similarities to real-world decisions, particularly for managers. Caplin et al. (2023) demonstrate the growing importance of decision making in the labor market, especially for managerial occupations. Many management decisions, such as hiring, retention, and promotion, involve binary judgments of other people (Benson and Shaw 2025). These decisions are increasingly augmented by algorithmic and AI recommendations (Chalfin et al. 2016, Dargnies et al. 2024). This creates a situation where managers must decide when to use their own judgment or to follow AI advice. This was the decision-making structure we aimed to emulate with our experimental setup. Of course, our task is a greatly simplified version of the complex and consequential decisions made by managers in the real world.

Image Selection: We utilized the IMDB-WIKI data set to source images for our experiment.[8] This data set is the largest publicly available collection of face images with age labels, and its size and diversity are useful for training machine learning algorithms. Our aim was to select 160 images from this data set so that the distribution of probability reports would be as even as possible, which would provide room for both humans and the AI to contribute in helping to identify the category of an image. Additionally, we wanted the AI predictions to be well calibrated over the selected images. To accomplish these objectives in a systematic way, we ran two pilots of 100 participants each. See the Online Appendix for more details on these pilots and how we used the results of the pilots to select 160 images.

Number of Rounds: In our two pilot sessions, we asked subjects to complete 100 rounds, and on average, participants finished the experiment within 15 minutes. Because performance did not decline over rounds, we increased the number of rounds to 160, allowing participants to complete the experiment within approximately 25 minutes on average.

Time Limit: There were two reasons why we introduced a fixed time limit of 60 seconds for participants to report a probability for each image. First, we wanted participants to remain focused on the experiment and not set aside the experiment to work on other tasks. Second, our pilots indicated that 60 seconds was sufficiently long to assess each image. In line with our pilots, this time limit was only reached in 0.23% of subject-round pairs in our experiment (275 out of 117,120 rounds). We exclude these responses from subsequent analyses, leaving 116,845 responses.

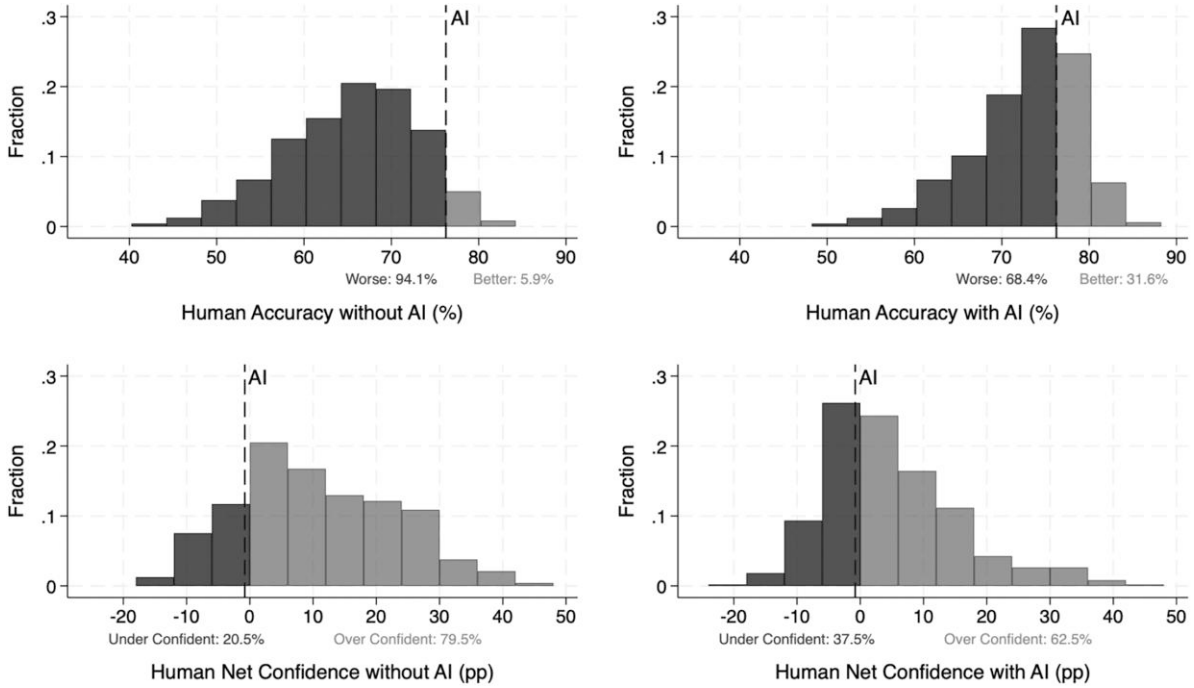
4. Results

4.1. Summary Statistics

We recruited 732 participants over two sessions, with 376 participants in the first session and 356 in the second session. Of those, 493 were assigned to the treatment of working with AI, and 239 were assigned to the control of never working with the AI. Summary statistics across both sessions are presented in Table 3 of Online Appendix A.1. This table shows that there was balance across control and treated participant characteristics.

Figure 1 plots the empirical distributions of ability and net confidence in the treated image block, split by subjects working with and without AI. The dotted lines indicate the performance of the AI. This illustrates

Figure 1. Empirical Distributions of Ability and Calibration Across Participants



several motivating facts. First, the AI has ability in the top 6% of human performers working alone, and working with the AI significantly improves average human performance in our task. However, there remains dispersion in human performance when working with AI, with 68% of treated subjects still performing worse than if they had followed the AI recommendations wholesale.[9] Finally, there is sizeable dispersion in humans' net confidence when working alone, with 79.5% of humans overconfident in their own ability and with individual confidence exceeding accuracy by 10.6 percentage points on average. We now turn to the estimation of our main Regression (3) to determine how much the heterogeneity in the gains from working with AI can be explained by individual-level ability and calibration.

4.2. Calibration and the Effects of Working with AI

Table 2 presents estimates from our main regression specification, Equation (3). We standardize all three skill measures (accuracy, calibration, IQ) to compare the relative heterogeneity of treatment effects across skills in the sample.

Column (1) shows the impact of AI assistance on accuracy in a simple bivariate regression.[10] AI improves participants' ability to accurately identify an image by 6.9 percentage points, an increase of more than 10% relative to the control mean of 65.5%. Column (2) adds participants' baseline accuracy with control images and an interaction between accuracy and treatment. Consistent with past work, we find that AI improves accuracy more for participants with low baseline skill—a one-standard-deviation increase in accuracy decreases the impact of AI assistance by 1.6 percentage points.

Column (3) adds baseline calibration and the interaction between calibration and treatment. Unlike the interaction with accuracy, we find that better-calibrated participants benefit more from AI assistance. A one-

Table 2. Regression: Skills and Working with AI

	Outcome: Accuracy (treated block)			
Skills (control block)	(1)	(2)	(3)	(4)
Treatment	6.93	6.27	6.16	6.27
	(0.56)	(0.47)	(0.47)	(0.47)
Accuracy		4.05	3.96	3.60
		(0.38)	(0.45)	(0.45)
Accuracy × *Treatment*		−1.57	−2.16	−1.83
		(0.50)	(0.57)	(0.57)
Calibration			0.19	0.11
			(0.44)	(0.43)
Calibration × *Treatment*			1.35	1.40
			(0.54)	(0.54)
IQ				1.17
				(0.43)
IQ × *Treatment*				−1.01
				(0.51)
Constant	65.46	65.96	65.99	65.88
	(0.48)	(0.39)	(0.40)	(0.39)
Observations	732	732	732	732

Notes. Observations are at the subject level. Robust standard errors are in parentheses. Skills are standardized within sessions.

standard-deviation increase in calibration increases the impact of AI assistance by 1.4 percentage points, or about 20% of the total treatment effect of 6.9 percentage points. Both interaction terms are statistically significant at the 5% level. Column (4) adds IQ and its interaction with treatment. Similar to the accuracy measure, we find that low-IQ participants benefit more from AI help. Adding IQ in column (4) does not meaningfully change the coefficients on calibration. Overall, we find that the impact of AI assistance is increasing in the baseline accuracy of participants' beliefs (calibration) but decreasing in the other two skills (accuracy and IQ).

To give a visual sense of the treatment effect variation, Figure 2 plots the estimated treatment effects for hypothetical participants with ability and calibration each one standard deviation above or below the mean, which we label, respectively, as *High* or *Low* ability or calibration. The coefficients and 95% confidence intervals in Figure 2 come from linear projections of the estimates from column (3) of Table 2, with the average treatment effect from column (1) added for clarity. Participants with low ability but accurate beliefs benefit the most—nearly 10 percentage points—from AI assistance. By contrast, participants with high accuracy but miscalibrated beliefs benefit the least.

Over- and underconfident participants may be harmed for different reasons. Underconfident participants may commit too many type I errors (following the AI when it is incorrect), whereas overconfident individuals may commit too many type II errors (failing to follow the AI when it is correct). In Table 5 of Online Appendix A.2, we repeat our main specification with net confidence instead of calibration and disaggregating by whether the AI prediction was correct. We find that the less confident are relatively more disadvantaged for images where the AI is ex post incorrect, whereas the more confident are more disadvantaged for images where the AI is ex post correct.

A remaining potential concern with our results is the presence of measurement error in our skills estimates. In a univariate setting, this would manifest as simple attenuation bias working against finding skill effects. However, in our multivariate context, Gillen et al. (2019) show that the attenuation bias in one covariate (in our case, individual ability A_i) could lead to a spurious effect of another covariate (calibration C_i) when the latter is correlated and thus picks up on the remnants of the true former effect.

We address this concern in two ways. First, if our measure of calibration were picking up on the effects of imperfectly measured ability, we would also expect to observe this spurious effect among control participants who never worked with AI; however, the coefficient on standardized calibration alone is economically and statistically insignificant across specifications in Table 2. Second, in Online Appendix A.3, we repeat the preceding analysis with an adaptation of the obviously related instrumental variables (ORIV) methodology of Gillen et al. (2019). Doing so, we find larger effects of calibration on the treatment effect, which, although less precise (an expected outcome of this procedure), remain statistically significant at conventional levels.

A final possibility is that our results depend on our choice of performance measures. In Online Appendix A.4, we repeat the main analysis, replacing our measure

Figure 2. Heterogeneous Treatment Effects by Skill-Calibration Groups

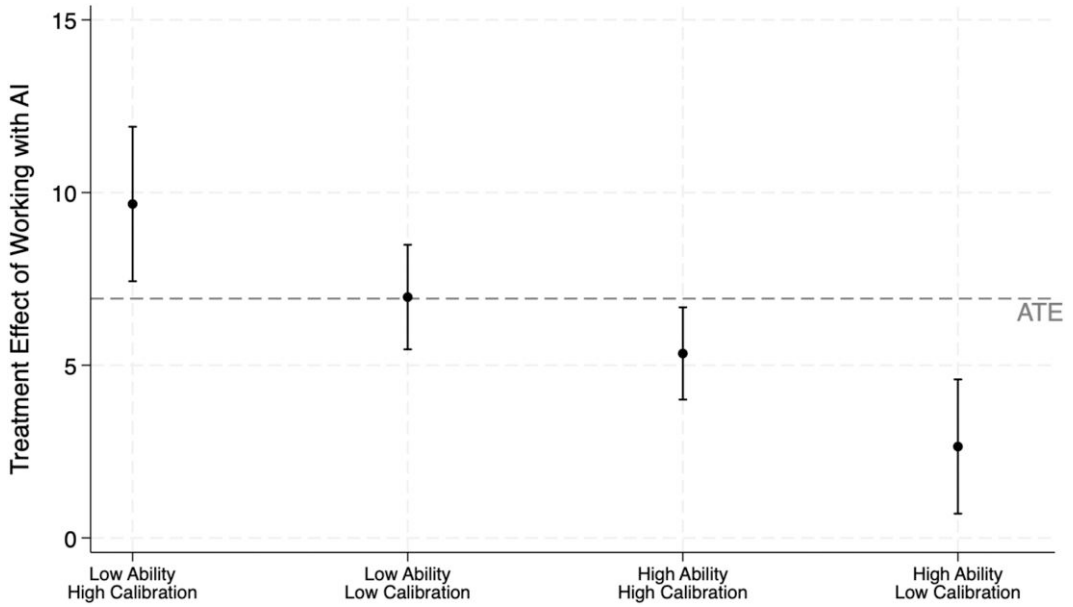
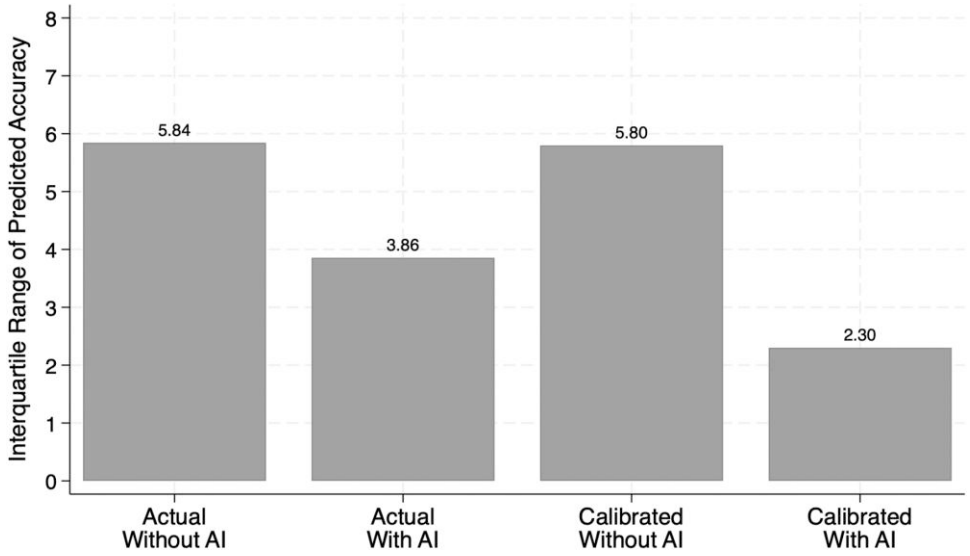


Figure 3. (Counterfactual) Productivity Gap



of ability (1) with AUC (Bamber 1975, Hanley and McNeil 1982) and our measure of calibration (2) with an alternative based on the functional form of Grether (1980). We replicate our main findings.

4.3. The Impact of Calibration on Performance Inequality

How does (mis)calibration affect the distribution of productivity when working with AI? Figure 3 presents the results of a counterfactual exercise where we estimate performance inequality across four scenarios. For ease of interpretation, we measure inequality using the interquartile range (IQR) of predicted performance across participants. In the first two scenarios, we estimate inequality with and without AI, given participants' *actual* (mis)calibration in the experiment. Specifically, we reestimate Equation (3) using unscaled measures of each skill, compute the predicted values, and then calculate the IQR implied by performance at the 25th and 75th percentiles. For the third and fourth scenarios, we compute the same statistics but with the coefficients on calibration taking on the values implied by a participant whose accuracy and confidence are exactly equal to each other.

We estimate an IQR of 5.84 when participants work without AI, compared with an IQR of 3.86 with AI, representing a 34% reduction in performance inequality from AI assistance. This is consistent with prior studies that find that AI reduces inequality by helping low-performing workers more (Noy and Zhang 2023, Autor 2024, Brynjolfsson et al. 2025).

Scenarios 3 and 4 repeat the exercise, but now, we simulate the impact of AI assistance in a counterfactual scenario where all participants are perfectly calibrated, meaning their accuracy and confidence take on exactly the same value. We estimate an IQR of 5.80 without AI. Thus, relative to the first scenario, improving calibration has no impact on accuracy when there is no AI assistance. However, with AI assistance, perfect calibration reduces the IQR to 2.30, which represents a 61% reduction in performance inequality relative to baseline (scenario 1).

Our counterfactual implies that if everyone were perfectly calibrated, working with AI would reduce performance inequality by an additional 27 percentage points beyond the 34-percentage-point reduction from AI assistance alone. This is because of the joint distribution of ability and calibration: the low-ability individuals who stand to gain the most from working with AI are also among the least calibrated. A natural set of questions that we leave to future work is if, how, and for whom individual calibration can be trained as a skill for the collaborative nature of work with AI.

5. Conclusion

In this paper, we introduce an experimental framework to measure the joint impacts of accuracy and calibration on the benefits of working with AI. We find that AI assistance improves overall prediction accuracy on average, but the extent and direction of the impact depend on whether participants are appropriately confident about their own ability. We also find that lower-ability participants who are well calibrated benefit the most from AI assistance, consistent with other studies finding that AI helps lower performers close the gap with coworkers with more expertise (Noy and Zhang 2023, Autor 2024, Brynjolfsson et al. 2025).

Finally, we perform a counterfactual analysis that shows that if miscalibration were eliminated, performance inequality would shrink nearly twice as much as

it does already with the introduction of AI assistance. In addition to showing the importance of calibration in performance inequality, this result might motivate the use of inventions targeting calibration. Among the characteristics that we show are related to performance when working with AI (baseline skill, IQ, and calibration), calibration appears to be a relatively strong candidate for policy and training. For example, training interventions have been shown to improve calibration (Lichtenstein and Fischhoff 1980, Benson and Önkal 1992, Haddara and Rahnev 2022), even with some short interventions lasting less than an hour (Gruetzemacher et al. 2024). In domains where conventional training interventions (sometimes referred to as "upskilling" programs) are costly or time-consuming, our results suggest that the combination of calibration training and AI assistance may be particularly valuable.[11]

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Endnotes

[1] For a review of literature on belief calibration and systematic departures, see Benjamin (2019).

[2] Tejeda et al. (2022) asked participants to classify fuzzy versions of images from the ImageNet database with (and without) the aid of AI recommendations, and they found that participants were more likely to accept AI recommendations when they were not confident but the AI was highly confident. Dargnies et al. (2024) found that participants who were more confident in their ability to make hiring decisions were less likely to use algorithmic advice. A broader motivation for studying belief calibration is provided by an extensive literature on overconfidence as a general trait (e.g., Moore and Healy 2008).

[3] This differs from Agarwal et al. (2023), where the subjective assessments of experts are aggregated to determine the ground truth.

[4] We adopt the convention that a bar indicates an analogous term as defined before, but when working with AI.

[5] To avoid confusion, we excluded images in which the individual was exactly 21 years old.

[6] For additional details on this model, see Online Appendix B.1.

[7] To this end, we selected images based on ages: 16 to 19 years old for the under-21 category and 23 to 26 years old for the over-21 category. We only know the year when the photo was taken, so to avoid the possibility of including subjects who were exactly 21, we excluded images of individuals who could have been 20, 21, or 22 years old.

[8] See https://data.vision.ee.ethz.ch/cvl/rrothe/imdb-wiki/ for details.

[9] Subjects repeat the AI recommendation exactly (i.e., within a percentage point) 6% of the time with AI and 2% of the time without AI.

[10] Table 4 replicates the main results but with demographic controls. We find very similar results. However, because demographic variables were missing for some participants, we present the main results without them.

[11] Another possibility is that working with AI could improve calibration, which would be an interesting question for future work.

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